

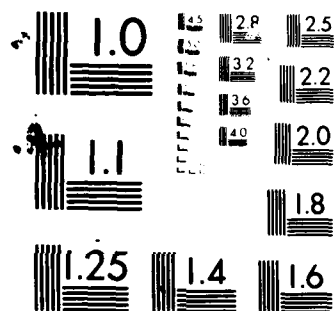
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Averaging Rules and Adjustment Processes:

The Role of Averaging in Inference

Lola L. Lopes

WHIPP #13

December 1981

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the direction of revisions in the Bayesian task that are well-accounted for by the simple adjustment strategy. Two additional results are also discussed: (1) a tendency for subjects in one experiment to evaluate sample evidence according to representativeness rather than according to relative likelihood, and (2) a strong recency effect that may reflect the influence of the internal representation of sample information during the judgment process.



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**Averaging Rules and Adjustment Processes:**

**The Role of Averaging in Inference**

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In the many years that psychologists have been studying human judgment processes, no two findings have emerged with greater empirical support than these: (1) Human judgments often appear to follow an averaging rule (Anderson, 1974), and (2) Judgments in Bayesian inference tasks are usually conservative relative to optimal judgments (Edwards, 1968).

Although these findings originated from two quite different research traditions (cf. Slovic & Lichtenstein, 1971), there is now considerable evidence that "averaging" and "conservatism" are not unrelated phenomena. In the present paper I discuss this evidence and describe a process model that attempts to explain why averaging occurs in the Bayesian task. Then I present two experiments that test an ordering prediction for the Bayesian task drawn from this process account. Last I discuss the averaging model in general and sketch its relationship to other kinds of algebraic judgment rules.

**Bayesian Inference and Averaging**

The Bayesian inference task is usually instantiated in terms of the "bookbag and poker chips paradigm" in which there are two well-specified hypotheses to be considered by the subject, usually involving populations of binary events. For example, there may be two bookbags, one containing 70 red poker chips and 30 blue poker chips (the "red bag") and another containing 30 red chips and 70 blue chips (the "blue bag"). In most experiments the experimenter ostensibly selects one of these bags at random and then draws

samples of one or more chips from it one or more times. These samples are shown to the subject, usually sequentially, and the subject is asked to indicate the strength of his belief about which bookbag was sampled using a rating scale of some kind (i.e., probability, odds, log-odds, etc.).

According to Bayes' theorem, the optimal response in such situations is found by multiplying the prior odds ratio for the two hypotheses ( $\Omega_0$ ) by the likelihood ratio of the sample given the two hypotheses (LR) to obtain the posterior odds ratio for the two hypotheses ( $\Omega_1$ ):

$$\Omega_1 = LR \cdot \Omega_0 \quad (1)$$

If more than one sample of data is given, the procedure is simply applied iteratively; the posterior odds ratio following sample  $n$  becomes the prior odds ratio for sample  $n+1$ :

$$\Omega_{n+1} = LR_{n+1} \cdot \Omega_n \quad (2)$$

In general, human responses to the Bayesian task are conservative relative to Bayesian responses; that is, human responses fall nearer neutral than Bayesian responses. Although various experimental manipulations can be used to influence the size of the conservatism effect (e.g., size of sampling unit, proportion of predominant chips in the population, response scale, payment for accuracy, and so forth), no simple manipulation has been successful in eliminating conservatism (but see Eils, Seaver, & Edwards, 1977, to be discussed below).

Three conceptually distinct sorts of explanations have been given for the conservatism effect. Misperception explanations locate the error in the process through which subjects estimate the likelihood ratio of the sample

data. In this view conservatism occurs because subjects underestimate the diagnostic impact of data, probably due to their having overly "flat" subjective sampling distributions, especially for large samples. Misaggregation explanations locate the error in the process through which subjects integrate the information from multiple samples into composite responses. Subjects ought to multiply, but somehow they don't. Response bias explanations treat conservatism as an artifact of the response scale that is engendered primarily by a tendency of subjects to avoid using extreme odds or probability judgments.

Although the Bayesian research literature has tended to treat the misperception, misaggregation, and response bias hypotheses as competitors, all three sources of error are likely to occur in Bayesian tasks. But misaggregation is probably the most important theoretically and practically because misaggregation appears to figure more prominently in producing conservatism (Edwards, 1968; Wheeler & Edwards, 1975) and also because it is probably easier to teach people improved methods for aggregating responses than it is either to improve their subjective impressions of the diagnostic impact of sample data or to remove their bias against using extreme responses (Eils, Seaver, & Edwards, 1977).

Averaging and conservatism. The first evidence to link averaging and conservatism came from experiments that showed that when subjects were asked to rate the probability that samples had been drawn from one of two statistically well-specified populations, their ratings were more often like estimates of the population proportion than they were like inferences from Bayes' rule (Beach, Wise, & Barclay, 1970; Marks & Clarkson, 1972, 1973; Shanteau, 1970, 1972). Shanteau hypothesized that subjects' behavior in such tasks could be modeled by an algebraic judgment rule in which the response  $R$  at serial position  $n$  is given by a weighted average of the scale values of the previous and current sample events:



$$R_n = \sum_{i=0}^n w_i s_i \quad (3)$$

In this equation the  $s_i$  are the values of the various stimuli and the  $w_i$  are weights that sum to unity. The term  $w_0 s_0$  signifies the weight and value of a neutral "initial impression." It should be noted that the averaging rule is conservative relative to the Bayesian rule since averages always lie within the range of their component stimulus values whereas Bayesian inferences must often be more extreme than any of their component values.

The averaging model does a generally good job of fitting subjects' inference judgments quantitatively. But even better evidence for the model comes from later experiments by Shanteau (1975) and by Troutman and Shanteau (1977) which show that presentation of neutral or non-diagnostic information causes subjects to revise their judgments toward neutral. This result is exactly what would be expected if subjects average the neutral information together with prior non-neutral information. But it is not allowed by Bayes' theorem, which specifies that neutral information ought to have no impact on prior judgments. That is, since the likelihood ratio for neutral information is one by definition, a judgment that follows neutral information ought (by Equation 2) to be numerically identical with the preceding judgment.

A different sort of evidence linking averaging and conservatism comes from experiments by Eils, Seaver, and Edwards (1977) that were aimed at the practical problem of aiding human performance in the Bayesian task. These authors hypothesized that since untutored subjects appear to average when they ought to multiply, they might be better at judging the mean log likelihood ratio for a set of samples than they are at judging the cumulative log likelihood ratio, which is the more typical Bayesian judgment.

The hypothesis was tested using two groups of subjects. One group rated "the average, rather than the total of, their certainty (Eils et al., 1977,

p. 6) and the other group rated their total or cumulative certainty. Then responses from both groups were transformed to log posterior odds form and individual regression analyses were performed for each subject comparing inferred log posterior odds to veridical log posterior odds. The analyses confirmed the hypothesis: although log odds inferred from average certainty judgments were now slightly radical, they were definitely closer to veridical than odds inferred from cumulative certainty judgments.

Averaging and serial adjustment processes. Although averaging rules have often been successful in accounting for judgment data quantitatively (cf. Anderson, 1974), little attention has been directed at finding out why averaging occurs qualitatively. The present research is based on the assumption that averaging results from the intrinsically serial nature of multiattribute judgment, both for tasks in which the information is actually presented serially (such as the typical Bayesian task) and for tasks in which the information is presented simultaneously but processed serially (such as the typical impression formation task). In either case, averaging is hypothesized to occur because subjects adopt an adjustment strategy in which they integrate new information into "old" composite judgments by adjusting the old composite value upward or downward as necessary to make the "new" composite lie somewhere between the "old" composite and the value of the new information (Lopes & Johnson, in press; Lopes & Oden, 1980; see also the research on anchoring and adjustment by Tversky & Kahneman, 1974). Such a process would be equivalent qualitatively to weighted averaging. But subjects would never need "compute" an average in any intentional sense of that term. Rather, averaging would simply emerge as a natural consequence of their adjustment strategy.

It is clear that any judgment process yielding averages will differ quantitatively from Bayes' theorem. But the hypothesized adjustment process

also differs qualitatively in a way that can be tested experimentally. To illustrate this qualitative difference, consider a subject who is asked to produce serial judgments about whether samples have been drawn from a 70/30 red/blue bookbag or a 30/70 red/blue bookbag. Assume that the rating scale is set up so that increased confidence in the "predominately red" hypothesis is associated with larger numbers. If the first sample favors the red bookbag moderately strongly (i.e., 5 red and 3 blue), the subject should make an initial judgment at some value favoring red. If the next sample also favors red, but more strongly (e.g., 7 red and 1 blue), the subject should notice this difference in strength and adjust his judgment upwards towards the value of the second sample. Note that such an adjustment is directionally in accord not only with the averaging rule, but also with the Bayesian rule. That is, since the likelihood ratio of the second sample is greater than one, the posterior odds actually do favor red more strongly than the prior odds.

If the two samples are reversed in order, however, so that the weaker evidence follows the stronger, the averaging rule and the Bayesian rule make qualitatively different predictions about the direction of the adjustment. Under the Bayesian rule the adjustment should still be upward: although the new sample is less favorable to red than the old, the likelihood ratio of the sample is still greater than one, so that the posterior odds should increase. Under the averaging rule, however, the adjustment should be downward (i.e., toward a more neutral value) since the value of the new sample is less favorable to red than the value of the judgment based only on the first sample. Thus, if subjects in the Bayesian task use the hypothesized adjustment strategy, errors should be evident in the direction of revisions when weaker evidence favoring a particular hypothesis follows stronger evidence favoring the same hypothesis.

### Experimental Tests of the Directional Hypothesis

Two experiments were run to test the hypothesis that subjects in the Bayesian inference task revise their posterior inferences in the normatively wrong (i.e., neutral) direction when a sample strongly favoring a given hypothesis is followed by a weaker sample favoring the same hypothesis. Since the experiments were essentially identical except for the stimulus designs, they are discussed together.

#### Method

Experimental tasks. Subjects in both experiments were asked to put themselves in the place of a machinist whose job was to make decisions concerning the maintenance of complex milling machines based on samples of parts produced by the machines. Subjects in Experiment 1 were asked to make judgments about whether machines needed maintenance or not. They were instructed that machines which were working properly were about as likely to produce parts that were a little too large as to produce parts that were a little too small. Broken machines, on the other hand, were described as tending to produce parts of which about 75% were a little too large and 25% were a little too small. Thus, in abstract terms, subjects in Experiment 1 were asked to decide between the hypothesis 50% large/50% small ( $H_{50/50}$ ) and the hypothesis 75% large/25% small ( $H_{75/25}$ ).

The task for Experiment 2 was similar except that subjects were asked to judge which of two maintenance procedures a machine needed. They were instructed that machines needing one procedure tended to produce parts of which about 75% were a little too small and 25% were a little too large.

Machines needing the other procedure, however, were described as tending to produce parts of which about 75% were a little too large and 25% were a

little too small. Thus, subjects in Experiment 2 decided between the hypothesis 25% large/75% small ( $H_{25/75}$ ) and the hypothesis 75% large/25% small ( $H_{75/25}$ ).

Stimulus designs. The stimulus design for Experiment 1 was a 7 x 7, first-sample x second-sample, factorial design in which the levels of both factors comprised the same seven sample distributions of large and small parts. The distributions were, for large and small parts, respectively: 3/7, 4/6, 5/5, 6/4, 7/3, 8/2, and 9/1. The design for Experiment 2 was also a first-sample x second-sample factorial design, but with nine levels on each factor. These levels were, for large and small parts, respectively: 1/9, 2/8, 3/7, 4/6, 5/5, 6/4, 7/3, 8/2, and 9/1.

Procedure. Subjects were run individually in sessions that took about 45 minutes for Experiment 1 and about 60 minutes for Experiment 2. At the beginning of the session subjects were brought into a sound proof booth and seated in front of a computer controlled video terminal. Subjects were then given general instructions about the nature of the task and shown how to read the stimulus display. A sample of a stimulus display for Experiment 1 is shown in Figure 1. At the top of the display is a box showing a sample of parts, 7 large and 3 small. Under the box is a notation showing that this is the first sample. At the bottom of the display is a response scale anchored at the left by the words "MILLING NORMALLY (50%)" and at the right by the words "MILLING TOO LARGE (75%)." The display for Experiment 2 was identical except that the response scale was anchored with "MILLING TOO SMALL (75% SMALL)" at the left and "MILLING TOO LARGE (75% LARGE)" at the right.

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Figure 1 about here  
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The procedure for each trial was identical. Subjects read the information for the first sample and then rated the degree of their belief as to whether the machine was milling normally or not (in Experiment 1) or whether the

machine was milling too large or too small (in Experiment 2). They made their ratings using a hand held response device to move the rating arrow (shown in the middle of the scale in Figure 1) along the response scale. When they finished their initial rating, subjects pushed a button on the response device. This caused the rating to be transmitted to the computer and also caused the first sample to be erased and replaced by a second sample of parts from the same machine. Subjects then revised their initial rating to account for the new sample and pushed the response button to transmit their final response to the computer. Finally subjects initialized the next trial by returning the response arrow to the middle of the scale.

In both experiments special precautions were taken to make sure that subjects understood the judgment task. In particular, the instructions emphasized that subjects should consider that the two samples within a trial were drawn independently from the same machine, and that samples from different trials were drawn from different machines. Subsequent debriefing of the subjects indicated that all of them had understood these instructions.

Subjects in both experiments were given 15 trials for practice and then were run through two replications of the stimulus design. Experimental trials within each replication were ordered randomly, but with the restriction that no sample appear as either first-sample or second-sample on two consecutive trials.

Subjects. The subjects for the two experiments were 41 and 39 student volunteers, respectively, who served for credit to be applied to their course grades in introductory psychology. In Experiment 1 subjects were all males; in Experiment 2 subjects were approximately evenly divided between the sexes.

### Results and Discussion

Ratings of single samples. In order to test the adjustment hypothesis,

it is necessary to determine at least roughly what subjective values the subjects attached to the various sample types. This can be done by looking at the responses subjects gave to the first sample of each stimulus pair. The data are given in Table 1, averaged over both subjects and replications. Ratings have been scaled to run between 0 and 1.

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Table 1 about here  
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For reasons that will be clear shortly, it is best to begin with Experiment 2. Basically, the results are very simple: ratings of the likelihood of  $H_{75/25}$  increased essentially linearly from 1/9 samples to 9/1 samples with 5/5 samples being rated as neutral. In fact, the close numerical correspondence between the ratings and the proportion of large parts in the samples suggests that subjects probably produced these initial ratings by the simple expedient of using the sample proportion as a judgmental "anchor" (Tversky & Kahneman, 1974). The results of Experiment 1 are more difficult to understand. In gross terms, there is no problem: samples of 3/7 through 6/4 were rated as supporting  $H_{50/50}$  and samples of 7/3 through 9/1 were rated as supporting  $H_{75/25}^1$ . But the data deviated from the norm ordinarily in that 5/5 samples were judged to be more supportive of  $H_{50/50}$  than either 3/7 or 4/6 samples.

Inspection of single subject data for Experiment 1 revealed clear individual differences in how subjects evaluated these samples. Most subjects (38 out of 41) could be assigned to one of three groups. The first group (15 subjects) ordered the samples appropriately, with 3/7 samples being taken as stronger evidence for  $H_{50/50}$  than 4/6 samples and these in turn as stronger evidence than 5/5 samples. These subjects will be called the "likelihood ratio" group since they appeared to judge the value of sample evidence in terms of the relative degree to which it supported the two hypotheses. The second group (12 subjects) ordered the samples in exactly inverse order to the norm: 5/5 samples were taken

to be the strongest evidence of  $H_{50/50}$  followed by 4/6 and 3/7 samples in that order. These subjects appeared to judge the samples according to how representative they were of a 50/50 generating process (Kahneman & Tversky, 1972). They will therefore be called the "representativeness" group. The third group (11 subjects) ordered the samples 5/5, 3/7, 4/6. This "mixed" group appeared to be influenced by representativeness only if samples were "perfectly" representative. Otherwise they seemed to rely on relative likelihood considerations.

The fact that many subjects in Experiment 1 appeared to prefer using representativeness rather than relative likelihood as the basis for evaluating samples causes no problems for testing the adjustment hypothesis other than making it necessary to perform separate tests for the various groups. But the result is generally problematical since misorderings of sample data have not been reported previously in the Bayesian literature and also since the effect did not occur either for Experiment 2 or for the samples supporting  $H_{75/25}$  in Experiment 1. It is therefore worth considering why the error occurred when it did.

One possibility is that subjects may have believed mistakenly that normal machines always produce 50/50 samples. But this is unlikely since subjects were instructed quite explicitly that normal machines do not always produce exactly 50% large parts, but sometimes produce more and sometimes less. A better possibility seems to be that the task violated the conventional semantics of what it means for a machine to be working normally. Certainly it is a bit odd linguistically to say that the best evidence for the normal functioning of a machine is that it produces a sample with an abnormally large number of small parts. Thus, subjects may have slipped unintentionally into using a hybrid strategy in which samples favoring  $H_{75/25}$  were evaluated with respect to both hypotheses whereas samples favoring  $H_{50/50}$  were evaluated only with respect to  $H_{50/50}$ . Of course, there is no way to be sure that this is what happened.



But if the explanation is correct, errors of this sort should not be found when the task of choosing between  $H_{50/50}$  and  $H_{75/25}$  is framed more neutrally, as in the conventional "bookbags and poker chips" format.

Adjustments for second samples. The adjustment hypothesis states that when subjects are given two pieces of evidence that favor the same hypothesis but to different degrees (hereafter called a "homogeneous sample pair"), their judged confidence in the hypothesis will increase if the stronger sample follows the weaker, but will decrease if the weaker sample follows the stronger. Bayes' theorem, in contrast, specifies that confidence should increase independently of the order of the samples. Thus, if the adjustment hypothesis is correct, directional errors in revision should occur more frequently for strong-weak sample orders than for weak-strong sample orders.

Table 2 gives the proportions of directionally correct adjustments (relative to the Bayesian norm) for strong-weak sample orders and weak-strong sample orders. These are pooled over sample pair, subjects, and replications. Since the subjects in Experiment 1 disagreed about the ordering of the sample evidence, their data have been divided into subgroups.

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Table 2 about here  
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The basic result is clear in all four comparisons: the data support the adjustment hypothesis. Adjustments were almost always made in the correct direction for weak-strong sample pairs and almost never made in the correct direction for strong-weak sample pairs,  $p < .01$  for all group differences.

Recency effects. The adjustment data above are qualitative in the sense that they do not show whether differences in sample order produced differences in the magnitudes of the final responses. Under the adjustment hypothesis, however, any of three quantitative effects might occur: (1) adjustments might be exactly sufficient to produce a true arithmetic "running average" of the

sample evidence in which case the final response data would show no effect of order; (2) adjustments might be insufficient for arithmetic averaging in which case final responses would be closer numerically to the first sample than ought to be the case, constituting a judgmental primacy effect; (3) adjustments might be over sufficient for arithmetic averaging in which case final responses would be closer numerically to the second sample than they ought, constituting judgmental recency. In either of the latter two situations order effects would occur for corresponding strong-weak and weak-strong sample pairs.

Final response data for homogeneous sample pairs are given in Table 3 organized according to the relative strength of the two samples. The data for Experiment 1 have been broken down according to how subjects ordered the sample types. For likelihood ratio subjects (but not for representativeness subjects or mixed subjects) there was a significant recency effect that appeared as a tendency for final responses to be more extreme when samples were ordered weak-strong than when they were ordered strong-weak,  $F(1,14) = 6.91$ ,  $p < .05$ . That is, for pairs that favored  $H_{50/50}$  weak-strong responses were smaller than strong-weak responses and for pairs that favored  $H_{75/25}$  weak-strong responses were larger than strong-weak responses. Thus, more extreme final responses were generated when the more extreme of two samples that favored the same hypotheses was presented in the second position. There was also a prominent recency effect for homogeneous pairs in Experiment 2,  $F(1,38) = 39.77$ ,  $p < .001$ .

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 Table 3 about here  
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Final responses for heterogeneous sample pairs are given in Table 4. Since the samples in these pairs favored different hypotheses, the data have been organized according to whether the ordered pair required adjustment toward  $H_{50/50}$  (leftward adjustment) or toward  $H_{75/25}$  (rightward adjustment). For

both experiments there was a strong recency effect manifested by a tendency for pairs requiring leftward adjustment to produce smaller final responses than equivalent pairs requiring rightward adjustment:  $F(1,40) = 10.40$ ,  $p < .01$  for Experiment 1 and  $F(1,38) = 27.89$ ,  $p < .001$  for Experiment 2. In other words, pairs in which the two samples favored different hypotheses produced final responses that favored  $H_{50/50}$  more strongly (i.e., were nearer 0) if the  $H_{50/50}$  sample was in the second position, and favored  $H_{75/25}$  more strongly (i.e. were nearer 1) if the  $H_{50/50}$  sample was in the first position. In fact, the effect was so strong in Experiment 2 that final responses for corresponding pairs tended to lie on opposite sides of the neutral point of the response scale.

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Table 4 about here  
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Recency effects have been reported previously in the Bayesian literature (Pitz & Reinhold, 1968; Shanteau, 1970, 1972) with the point being stressed that order effects are not allowed by the Bayesian model. But order effects are also important for what they suggest about the underlying representation and use of task information during the judgment process. In the present case, the tendency for subjects in Experiment 2 to make their final responses lie on the side of the response scale favored by the second sample suggests that subjects may have represented task information internally in such a way that they were unable to appreciate simple quantitative relationships between successive samples. For example, for the pair (1/9, 8/2) subjects initiated their responses at an average value of .09 following the 1/9 sample and then adjusted to .59 following the 8/2 sample. Thus, subjects expressed mild confidence in  $H_{75/25}$ . But subjects ought to have favored  $H_{25/75}$  since the sample favoring  $H_{25/75}$  was more extreme than the sample favoring  $H_{75/25}$ .

Why do such errors occur? Certainly they would not be expected if subjects

were assumed to compute running sample differences. But if subjects concentrate on adjusting for the difference between the new sample and the old response, they may be unlikely to compare the samples directly and hence may fail to notice which of the samples should dominate the response. In other words, once stimulus information has been transformed into response mode, all further processing of and adjustments to the interim response may be based solely on that transformed value rather than on some more literal representation of the raw stimulus information.

### Discussion

The experiments reported here demonstrate that the adjustment processes used by subjects in the Bayesian task are consistent with an averaging rule and inconsistent with the multiplicative rule specified by Bayes' theorem. In particular, the present experiments supplement the earlier work of Shanteau (1975) and Troutman and Shanteau (1977) by demonstrating qualitative adjustment errors for diagnostic samples. But the primary purpose of the paper is not so much to support the averaging rule, as it is to suggest the psychological processes that produce averaging.

### Adjustment Mechanisms and Algebraic Judgment

The main thesis of the present paper is that subjects in Bayesian inference tasks average when they ought to multiply because they produce their judgments by using a serial adjustment strategy that just happens to be equivalent to averaging. But subjects in judgment tasks don't always average. In fact, there are some tasks such as judging the worth of gambles (Anderson & Shanteau, 1970; Shanteau, 1974; Tversky, 1967) and judging the likelihood of joint events (Beach & Peterson, 1966; Lopes, 1976; Shuford, 1959) in which subjects seem to

multiply. Why do they average in one case and not the other?

The position taken here is that averaging and multiplying reflect basically similar adjustment strategies operating in different task environments. Tasks that produce averaging usually call for bidirectional adjustments in which the judgment is sometimes increased and sometimes decreased, as is the case in the Bayesian task for heterogeneous sample pairs. Tasks that produce multiplying, on the other hand, usually call for adjustments which are unidirectional and most often downward. For example, one can judge the value of a gamble by beginning with the value of the prize to be won and then adjusting downward in proportion to the probability of winning (Lopes & Ekberg, 1980). Similarly, one can judge the probability of a joint event by beginning with the probability of one event and then decreasing this in proportion to the probability of the other event (Lopes, 1976). Both processes involve downward adjustment and both are equivalent to multiplying.

A basic corollary of the present view is that subjects in judgment tasks do not "choose" judgment rules in the sense that they decide how information ought to be combined. Rather they choose adjustment processes that seem to "fit" the task, both in terms of the ease with which the process can be executed mentally and in terms of the degree to which the process generates plausible judgments. Usually this works out reasonably well. But now and then the adjustment strategy that seems to subjects to fit the task best will turn out to be normatively inappropriate.

It is also clear that the judgment rule subjects employ for judging A from B and C will not necessarily bear a logical relationship to the rules they employ for judging B from A and C or C from A and B (Anderson & Butzin, 1974; Graesser & Anderson, 1974). For example, Graesser and Anderson asked subjects to judge income, generosity, or expected gift size from information about the other two. The data revealed that although income and generosity were combined

multiplicatively in judgments of expected gift size, there were no corresponding dividing relations between gift size and generosity for judgments of income, or income and gift size for judgments of generosity. Instead, these latter judgments seemed to follow some sort of subtracting rule. Thus, the human judgment system appears to comprise a set of individual judgment processes that may sometimes correspond to simple arithmetic operations, but that tend to be both non-reversible and largely unrelated to one another.

#### Judgment Processes and Memory Processes

The present adjustment model makes two fundamental assumptions about the characteristics of the judgment apparatus. First, it assumes the existence of an internal quantitative dimension that is reasonably continuous, and second, it assumes that this dimension is directly available for manipulation by the judge. Judgment itself is hypothesized to occur as quantitative information is extracted from stimuli one by one (the evaluation process) and used to modify the current judgment (the adjustment process).

A similar model was proposed some years ago by Anderson and Hubert (1963) to account for the fact that order effects in impression formation differ from the effects that would be expected if the impression depended on verbal memory. They concluded that judgments in the impression task involve "a memory process which is distinct from, and not dependent on, the immediate verbal memory for the adjectives just heard (p. 386)."

An important feature of the Anderson and Hubert (1963) model is that it allows the possibility that judgment processes may sometimes be insensitive to certain interactive relationships among stimuli. For example, a judge might fail to notice that the current piece of stimulus information is inconsistent or redundant with previously integrated information. Or, as may have actually occurred for heterogeneous pairs in Experiment 2, a judge might produce a

response to a sequence of stimuli that differs qualitatively from the response that would be made if the stimuli were retrieved from verbal memory and combined in raw form before being submitted to the evaluation process.

Quantitative judgment is both commonplace and fundamental, and much is known about the content and algebraic structure of judgment data. But relatively little is known about actual judgment processes. This is true in part because judgment processes do not yield gracefully to the sorts of experimental manipulation that have been useful in other cognitive domains. But the present experiments as well as those by Shanteau (1975), Troutman and Shanteau (1977), and Anderson and Hubert (1963) suggest that the study of temporal effects in judgment tasks such as order effects and adjustment effects may provide deeper insights into the cognitive mechanisms that are invoked when people make judgments.

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## Footnotes

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<sup>1</sup>This happens, in fact, to be in accord with the actual likelihood ratios of the samples.

Table 1  
Mean Ratings of First Samples

Experiment 1		Experiment 2	
Sample L/S	Rating	Sample L/S	Rating
3/7	.20	1/9	.08
4/6	.23	2/8	.16
5/5	.16	3/7	.24
6/4	.45	4/6	.31
7/3	.73	5/5	.49
8/2	.85	6/4	.67
9/1	.94	7/3	.74
		8/2	.82
		9/1	.91

Note. A rating of 0 indicates complete confidence in  $H_{50/50}$  for Experiment 1 and  $H_{25/75}$  for Experiment 2. A rating of 1 indicates complete confidence in  $H_{75/25}$ .

Table 2  
Proportion of Directionally Correct Adjustments  
for Homogeneous Sample Pairs

	Strong- Weak	Weak- Strong	F <sub>diff</sub>	(df)
Experiment 1				
Likelihood ratio group	.35	.89	32.17*	(1,14)
Representativeness group	.30	.86	76.14*	(1,11)
Mixed group	.29	.92	115.74*	(1,10)
Experiment 2				
	.25	.92	165.69*	(1,38)

\* p < .01

Note. In Experiment 1 there were 18 strong-weak and 18 weak-strong comparisons per subject and in Experiment 2 there were 24 strong-weak and 24 weak-strong comparisons per subject. A list of the sample pairs is available in Table 3.

Table 3  
Final Ratings of Homogeneous Pairs

Experiment 1													
Likelihood group				Representativeness group				Mixed group		Experiment 2			
Sample Pair	Weak-Strong	Strong-Weak		Weak-Strong	Strong-Weak		Weak-Strong	Strong-Weak		Sample Pair	Weak-Strong	Strong-Weak	
Favor $H_{50/50}$													
(3/7,4/6)	.16	<	.20	.20	<	.24	.25	<	.27	(1/9,2/8)	.07	<	.10
(3/7,5/5)	.16	<	.23	.16	<	.18	.19		.19	(1/9,3/7)	.07	<	.15
(3/7,6/4)	.23	<	.30	.29		.26	.48		.38	(1/9,4/6)	.10	<	.22
(4/6,5/5)	.25		.25	.13	<	.14	.23		.20	(2/8,3/7)	.14	<	.17
(4/6,6/4)	.28	<	.33	.22	<	.25	.39	<	.40	(2/8,4/6)	.16	<	.25
(5/5,6/4)	.29	<	.36	.21		.18	.35		.25	(3/7,4/6)	.21	<	.27
Favor $H_{75/25}$													
(7/3,8/2)	.88	>	.80	.83	>	.81	.78	>	.75	(6/4,7/3)	.76	>	.70
(7/3,9/1)	.96	>	.85	.84		.85	.81	>	.79	(6/4,8/2)	.82	>	.75
(8/2,9/1)	.96	>	.92	.87	>	.86	.83	>	.81	(6/4,9/1)	.91	>	.77
										(7/3,8/2)	.86	>	.82
										(7/3,9/1)	.92	>	.85
										(8/2,9/1)	.93	>	.89

Note. Recency is indicated for pairs favoring  $H_{50/50}$  and  $H_{25/75}$  by weak-strong ratings that are smaller than strong-weak ratings. Recency for pairs favoring  $H_{75/25}$  is indicated by weak-strong ratings that are greater than strong-weak ratings.

Table 4  
Final Ratings of Heterogeneous Pairs  
for Leftward and Rightward Adjustment

Experiment 1			Experiment 2		
Sample Pair	Leftward (toward H <sub>50/50</sub> )	Rightward (toward H <sub>75/25</sub> )	Sample Pair	Leftward (toward H <sub>25/75</sub> )	Rightward (toward H <sub>75/25</sub> )
(3/7,7/3)	.35	< .46	(1/9,6/4)	.23	< .46
(3/7,8/2)	.42	< .54	(1/9,7/3)	.26	< .52
(3/7,9/1)	.44	< .64	(1/9,8/2)	.28	< .59
(4/6,7/3)	.43	< .48	(1/9,9/1)	.32	< .63
(4/6,8/2)	.46	< .58	(2/8,6/4)	.28	< .51
(4/6,9/1)	.54	< .68	(2/8,7/3)	.31	< .59
(5/5,7/3)	.45	.45	(2/8,8/2)	.36	< .61
(5/5,8/2)	.52	< .60	(2/8,9/1)	.37	< .72
(5/5,9/1)	.55	< .66	(3/7,6/4)	.35	< .52
(6/4,7/3)	.64	.64	(3/7,7/3)	.37	< .58
(6/4,8/2)	.72	< .77	(3/7,8/2)	.41	< .64
(6/4,9/1)	.73	< .82	(3/7,9/1)	.44	< .73
			(4/6,6/4)	.43	< .57
			(4/6,7/3)	.45	< .65
			(4/6,8/2)	.49	< .70
			(4/6,9/1)	.51	< .75

Note. The terms "rightward" and "leftward" refer to the end of the response scale favored by the second sample. Leftward ratings smaller than corresponding rightward ratings indicate recency.

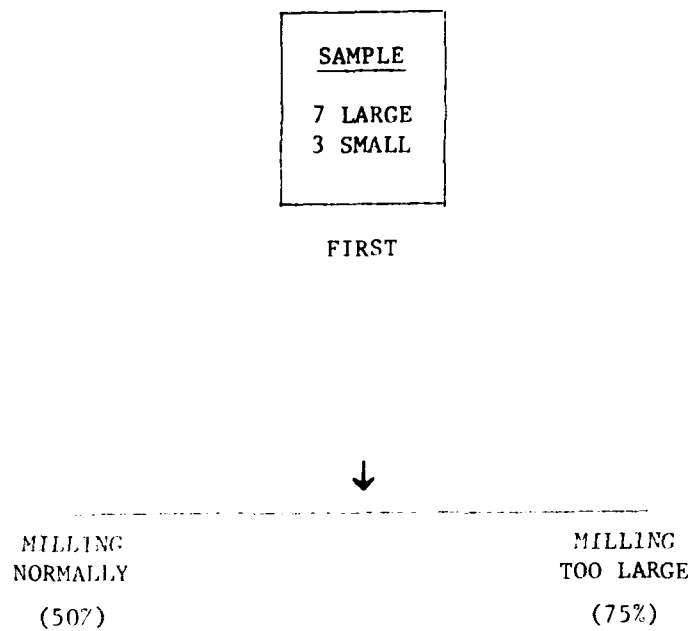


Figure 1



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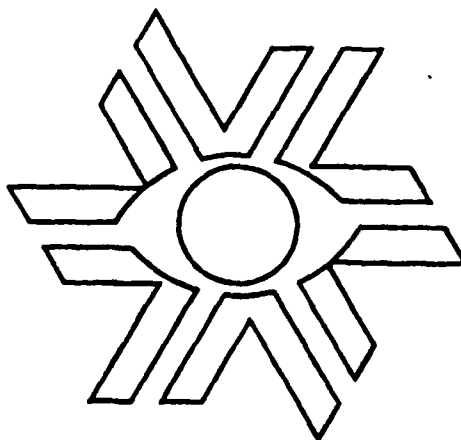
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